

# THE PSYCHOLOGICAL BULLETIN

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## THE PUZZLE OF COLOR VOCABULARIES.

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It was Gladstone<sup>1</sup> who first, in 1858, called attention to the rather extraordinary vagueness of early color nomenclature. Collating from the Iliad and Odyssey the passages which referred to color, he found such uncertainty and inconsistency in the application of color names as to lead him to deny to the Greeks of Homeric times any clear notions of color whatever. "I conclude," he says, "that the organ of color and its impressions were but partially developed among the Greeks of the heroic age."

This hypothesis of Gladstone was made more precise and given a definite evolutionary character by Geiger,<sup>2</sup> who, extending the study to many ancient literatures, found them all defective in the same respect; they all showed a lack of any clear term for blue, and the oldest of them had also none for green. Speaking of the Vedas, Geiger says:<sup>3</sup>

"These hymns, consisting of more than 10,000 lines, are nearly all filled with descriptions of the sky. Scarcely any subject is more frequently mentioned; the variety of hues which the sun and dawn display in it, day and night, clouds and lightnings, the atmosphere and the ether, all these are with inexhaustible abundance exhibited to us again and again in all their magnificence; only the fact that the sky is blue could never have been gathered from these poems by any one who did not already know it himself. . . . The Bible, in which, as is equally well known, the sky or heaven plays no less a part, see-

<sup>1</sup> *Studies on Homer and the Homeric Age*, III., 457-499.

<sup>2</sup> *Contributions to the History of the Development of the Human Race*, translated by D. Asher, London, 1880.

<sup>3</sup> *Ibid.*, pp. 50-61.

ing that it occurs in the very first verse, and in upwards of 430 other passages besides . . . yet finds no opportunity either of mentioning the blue color.

"The color *green* is met with in antiquity one stage farther back than the blue. . . . The ten books of Rigveda hymns, though they frequently mention the earth, no more bestow on it the epithet green than on the heavens that of blue."

In the very earliest literary remains, according to Geiger, there is not even a name for pure yellow, though there is one for golden or reddish yellow; red appears more firmly entrenched than yellow. And, by aid of etymology, the author believes it possible to go back of even the earliest literature, and "arrive at a still earlier stage, when the notions of black and red coalesce in the vague conception of something colored."

Following his general doctrine that the development of the human mind can be traced by aid of the history of language, Geiger concludes that in an early stage of human development, only a vague sense of indefinite color existed; that red first took on the character of a definite sensation, and that the other colors followed in the order of the spectrum.

The views of Geiger were warmly espoused by Magnus,<sup>1</sup> who, besides attempting to trace a gradual evolution in the use of color names in Greek literature, took the important step of examining, on a wide scale, both the color vocabularies and the color sense of existing primitive peoples. As a result of a questionnaire, with a set of colors to be named and distinguished, sent out to traders and missionaries, Magnus discovered that most primitive tribes possessed a color nomenclature which was incomplete in about the same way as that of Homer or of the Vedas. But he also found that the limits of color vision were the same among these tribes as among Europeans. They could see and distinguish all the colors from red to violet, though usually they did not possess names for them all. This discrepancy between color vision and the color vocabulary was in itself an important discovery, since it betrayed the weak foundation of the philological method.

This same discrepancy, however, was not only a discovery, but also a problem. Why should color nomenclature not be fairly adequate to the development of the color sense, and why should it be so much further advanced among some peoples than among others? In particular, the uniform character of the deficiency constituted a problem.

<sup>1</sup>*Die geschichtliche Entwicklung des Farbensinnes.* Leipzig: 1877. *Untersuchungen über den Farbensinn der Naturvölker.* Jena: 1880.

Magnus was able to establish definite laws governing the growth of color vocabularies. Color nomenclature begins, almost always, with red, and spreads to the other colors in spectral order, usually however skipping transitional colors such as orange, blue-green and violet. Practically every language has a name for red; nearly all have a name for yellow; but comparatively few have a conventional word for green, and still fewer have one for blue. Neighboring colors, especially green and blue, are sometimes called by the same name. Where a name for blue is absent, a blue, or at least a saturated blue, is very commonly called by the name which seems primarily to designate black or dark or dull. These results of Magnus's inquiry, which have been generally confirmed by later observations, certainly set a pretty problem in folk psychology.

The observations of Rivers, on both the color vocabulary and the color sense of several primitive groups, are probably the most accurate and important which we possess. He has examined four peoples by a constant method: the Egyptian peasants,<sup>1</sup> the Papuans of Torres Straits,<sup>2</sup> the Uralis and Sholagas of India,<sup>3</sup> and the Todas of Southern India<sup>4</sup>; and also a number of whites (English) for purposes of comparison.

Among the natives of Torres Straits he found four stages in the development of a color vocabulary represented in different islands. Some of these stages he has also found exemplified in Egypt and India. "In the lowest there appears only to be a definite term for red apart from white and black; in the next stage there are definite terms for red and yellow, and an indefinite term for green; in the next stage there are definite terms for red, yellow and green, and a term for blue has been borrowed from another language; while in the highest stage there are terms for both green and blue, but these tend to be confused with one another." This highest stage differs but slightly from that of popular language in Europe.

To complete this account of the different stages of color nomenclature, it should be added that some languages have no conventional color names at all. Some American Indian languages show this peculiarity;<sup>5</sup> the color of any presented object will be said to be like

<sup>1</sup> *Journal of the Anthropological Institute*, 1901. Vol. 31, p. 239.

<sup>2</sup> Reports of the Cambridge Anthropological Expedition to Torres Straits, 1901. Vol. 2, p. 48.

<sup>3</sup> *Bulletin Madras Government Museum*, 1903. Vol. 5, p. 3.

<sup>4</sup> *Brit. Journ. of Psychol.*, 1905. Vol. 1, p. 326; this article contains a brief summary of all his results on color sense; see also his article in the *Pop. Science Monthly*, 1901. Vol. 59, p. 48.

<sup>5</sup> So I am informed by Professor F. Boas.

that of some other object, and such comparisons are often very accurately made; but there is an absence of fixed usage as to what objects shall constitute the standards for comparison, so that color language remains in a fluid state. Rivers observed much the same fact in the naming of certain colors by the natives of Torres Straits. Probably we shall be right in recognizing several stages in the establishment of a color name: in the first stage comparisons are fluid and there is no fixed usage; in a later stage usage centers about some one comparison, so that all objects having a certain (approximate) color are said to have the color of one particular object; in a later stage, as described by Wundt,<sup>1</sup> the name becomes abstract in the sense that the object is no longer thought of when its name is used as the designation of a color; and later still the name may chance to become obsolete as applied to the object and remain exclusively as the name of the color. Obsolescence of the original usage of the name is probably a purely incidental feature, but is of interest, when it occurs, as showing antiquity of the color meaning.

Fixity of usage depends probably on frequency; where the need for designating a certain color, or range of colors, is infrequent, the fluid condition of color designation will be adequate, and no occasion will arise for stereotyping the name or for dissociating its color meaning from its reference to a particular object. It is evident that growing fixity of usage by no means indicates a finer sensitivity to differences in color, since each stereotyped color name covers a considerable range of discriminable colors. For this reason the original fluid method of color designation persists alongside of the fixed usage, and is employed especially by those who need to designate colors more precisely than is accomplished by the use of the conventional names.

Another fact which has not hitherto been introduced into the discussion of color vocabularies concerns the richness of European languages in names for different parts of the spectrum. The fact is that the English language, for example, is much richer in abstract names for the colors toward the red end of the spectrum than for colors toward the violet end. The following list includes all the color names which I have been able to find in modern English, in which the color reference is thoroughly dissociated, in common usage, from reference to any specific object.

For the reds and yellows and their various shades: red, ruddy, rubicund, russet, roan, auburn, carmine, crimson, scarlet, brown, bay, sorrel, dun, yellow, tawny, sallow, lurid. (To these might be added

<sup>1</sup>*Völkerpsychologie*, 1900, Bd. I, Th. 2, pp. 512-515.

buff, maroon, vermilion, and perhaps such words as magenta, since the objects to which these names primarily refer are not known by most persons who use the names.)

For the greens, blues and violets: green, verdant, blue, azure, purple, livid.

To the fact that names for red and yellow develop first in the history of a language should therefore be added the fact that they predominate in the European languages.<sup>1</sup> It is further to be noted that many of the names for shades of red and yellow are applied mostly to men and animals. Apparently there is some special demand for names of animal colors. Magnus reports that the Kaffirs have over thirty words and expressions to designate the colors and markings of cattle; and regarding another people of herdsmen, the Ovaherero or Damara of Southwest Africa, his informant makes the following suggestive statement:<sup>2</sup>

"Colors that coincide with those of cows, sheep and goats, they name without difficulty; but whatever is not a color of cattle, particularly blue and green, they cannot name, although they can distinguish them from the other colors, and when necessary use foreign words to designate them. . . . Those who have not come into contact with foreign culture and foreign names cannot name green and blue, and think it highly amusing that there should be names for these colors." It would indeed be ridiculous to have names for colors simply because the colors were distinguishable.

With these facts of color language before us, we may turn to the problem of explanation. At the outset, it is clear that no such view as that of Geiger can longer be entertained. The absence of a name for a sensory quality does not point to the absence of the quality. The case of smell is particularly convincing, for certainly many odors are vivid experiences, likely to attract the attention and be, relatively, isolated from a total sensory complex, and often they are practically significant; and yet there is an almost complete absence of abstract odor names from all languages. Taste nomenclature, though better defined

<sup>1</sup> This same predominance of red and yellow appears in the Greek and Latin languages. (See W. Schultz, *Das Farbenempfindungssystem der Hellenen*. Leipzig: 1904, pp. 83, 95.) The Greek was superior to English in possessing well-marked names for yellowish green (*χλωρός*) and for a more bluish green (*πράσινος*). It also, apparently, employed several names for blue, distinguishing light from dark blue; but the exact usage is here difficult to determine. *Κυανωίς* was apparently applied both to deep blue and to dark gray objects, and this is one of the 'confusions' which have led repeatedly to the conclusion that the Greeks were deficient in the color sense.

<sup>2</sup> Magnus, *Farbensinn der Naturvölker*, p. 9.

than that of smell, presents some striking incongruences with taste sensation, as has been shown by Myers<sup>1</sup> and by Chamberlain.<sup>2</sup> Many languages do not possess four words to correspond to the four tastes (which, it can hardly be doubted, are the property of all races of men), and even where the language is complete in this regard, common usage often employs one taste word to refer to substances of discrete tastes. In its most rudimentary condition, a taste vocabulary consists simply of words for well-tasting and ill-tasting; and in more developed languages this affective basis of taste nomenclature is still visible. Sweet and mildly saline are often called by one name; and sour and bitter by one name. Even Scotch villagers, examined by Myers, described a weak solution of quinine as having 'a sort of sour' or a 'sort of salty' taste. It is the exception, rather than the rule, when any sensation, either elementary or complex, receives a special name.

Magnus, as the result of his inquiry, was forced to abandon the doctrine of a close correspondence between color sense and the color vocabulary; but it still seemed to him impossible to explain the deficiency of names for green and blue without assuming some underlying sensory defect; and he is inclined to the view that these sensations, though present in primitive folk, were perhaps of recent development and certainly less vivid and impressive than to European eyes. Their lack of vividness, he supposed, was the cause of their remaining nameless.

This more moderate form of the Gladstone-Geiger hypothesis, though practically obsolete for two decades, has come into renewed prominence through the support of Rivers. Though this author admits the existence of other factors in the development of color language, he regards a lack of vividness of the sensation of blue as of great importance. This conclusion seems to him to be indicated by the results of his measurements of the sensitiveness of different peoples to colors. His measurements determined the faintest tint of red, yellow and blue which could be recognized and named. His results on four primitive peoples, as compared with the English, show average thresholds for the five groups which are in fairly close agreement, with the following exceptions. The English have a much lower threshold for yellow and for blue than any of the more primitive groups; and one of his primitive groups (the Murray Islanders) show a much lower threshold for red than either the English or any of the remaining groups.

<sup>1</sup> 'The Taste-names of Primitive Peoples,' *Brit. Journ. of Psychol.*, 1904, Vol. I., p. 117.

<sup>2</sup> 'Primitive Taste Words,' *Amer. Journ. of Psychol.*, 1903, Vol. 14, p. 146.

The only one of these differences which is emphasized by Rivers is the greater sensitiveness of the English for blue. This seems to him of importance as showing that the absence of a name for blue is actually associated with a certain degree of insensitiveness for this color. The low threshold of the English for *yellow* does not impress him as significant, and yet, though it is less marked than their low threshold for blue, it cannot be explained by the probable error of his observations, but is, on the contrary, rather striking. It creates some difficulty in the way of using the facts in explaining color nomenclature, since the name for yellow is present, though the sensitiveness to that color is comparatively blunt.

A more serious difficulty with Rivers' results is that so much depends on the group of 41 educated Englishmen whose average gives the basis for comparison.

The individuals in this group differed greatly from each other in sensitiveness to the colors. Rivers ascribes this variability, in case of red, to the probable presence in the group of some individuals with a tendency to red-green blindness — though demonstrably color blind individuals were of course excluded — and in the case of blue, to the possibility that some of his Englishmen helped out sensation by inference, calling a glass blue when they did not detect either of the two more easily recognized colors. It is possible, accordingly, that the English threshold for red is too high, and for blue too low, to compare fairly with the thresholds of the primitive groups; and, if so, the corrected result would read simply that the English were able to recognize and name fainter tints than the other races tested. Such a result might well be due to a better adaptation of the educated subjects to the conditions of the test, and to their greater habituation to analyzing and naming pale colors.<sup>1</sup>

Rivers himself accepts the low sensitivity of his primitive groups to blue as a well-established fact, and offers alternative explanations for it. One of his suggestions is new and deserves careful attention. "The Murray Islander," he says, "differs from the Englishman in two

<sup>1</sup> That this statement represents the true state of the case, I am strongly inclined to believe, on the basis of tests of several more or less primitive peoples made by Dr. Bruner and myself at the St. Louis Exposition. Publication of the full results of our work has been unduly delayed; and I will simply state that our test called for fine discrimination of color tones, instead of the recognition of pale tints, and is therefore, in strictness, not comparable with that of Rivers. At the same time, we found, clearly enough, that all the primitive peoples were inferior to white Americans in this test, but that the inferiority was no more marked in the blues than in the reds and yellows. Some of these primitive people, however, possess no names for green and blue.

important respects; he is more primitive and he is more pigmented, and his insensitiveness to blue may be either a function of his primitiveness or of his pigmentation. In other words, it is possible that his insensitiveness may depend on the lack of development of some physiological substance or mechanism, which acts as the basis of the sensation blue in ourselves, or it may only depend on the fact that the retina of the Papuan is more strongly pigmented than that of the European. There is some reason to think that this latter factor is the more important. We know that the macula lutea in the retina, which contains the region of most distinct vision, is pigmented, and that as a consequence of the reddish-yellow color of its pigment, blue and green rays are more strongly absorbed than red and yellow; we have reason to believe further that the macula of dark races is more pigmented than that of ourselves. The consequence would be that, in dark races, blue and green would be more strongly absorbed, and consequently there would be a certain degree of insensitiveness to these colors, as compared with red and yellow."<sup>1</sup>

This suggestion regarding retinal pigmentation is the more deserving of attention since Lindsay Johnson<sup>2</sup> has reported his ophthalmoscopic examinations of the fundus oculi in various races, and shown that the color of it ranges from orange-red in fair-haired Europeans to dark chocolate in the negro. What effect these differences in pigmentation should have on color vision, however, remains to be worked out. Meanwhile, this suggestion by no means contains a full explanation of the peculiar order of growth of color vocabularies, since this order has apparently been the same in light-skinned as in dark-skinned races. Even the Welsh language of today, according to Rivers, has no word for blue.<sup>3</sup> The late appearance of names for green and blue is too wide-spread a phenomenon to be explained in terms of racial differences.

Now, finally, to make an attempt at a positive explanation of the puzzle of color vocabularies. The matter now appears to me — after several years of consideration — rather simple and devoid of psychological interest, except perhaps from its bearing on the methods of folk psychology. We may fairly assume, with Wundt,<sup>4</sup> that abstract color names are of relatively late introduction into any language, and that they developed out of names for colored objects; so that the question is primarily regarding that hardening and dissociation of

<sup>1</sup> *Pop. Science Monthly*, 1901. Vol. 59, p. 52.

<sup>2</sup> *Philos. Trans. Roy. Soc. London*, 1901, ser. B, Vol. 194, p. 1.

<sup>3</sup> *Op. cit.*, p. 48.

<sup>4</sup> *Loc. cit.*

linguistic usage of which mention was previously made. What requires explanation is rather the development of this fixity of usage than its absence; some need for it must be pointed out, and this need must, undoubtedly, include frequency of reference to the color.

It is further evident that the color implication of the name of an object would never be dissociated from the whole connotation of the name, if there existed no other object of similar color, to which there was frequent need to refer. A color name can scarcely develop except where there are a variety of objects of the same general color.

A further necessity is brought into view by considering the function of color in practical and primitive life. The color of an object is not a practically important character, except as a ready means, and often the readiest means, of identifying the object and distinguishing it from its background. It is where color serves as the mark of an important object, or condition of an object, that a color name would be most likely to develop. But a color name need not always develop even in these circumstances, for practical tendencies lead us to pass quickly from a sign to the thing signified, and to speak of the thing signified rather than of the sign. We speak of a gray patch in the sky as a cloud, of a red spot on the skin as blood; we speak of a berry as ripe rather than red, of a knife as rusty rather than brown, of meat as well done or underdone rather than as brown or red. In such cases the color is the mark by which the condition of the thing is known, but what is named is the condition rather than the mark.

Such considerations make clear the lack of a need for color names, and account for the widespread poverty of languages in such names. They also help to explain why red and yellow are so much more generously supplied with names than green and blue.

Green and blue, in nature, are predominantly background colors, while red and yellow are usually the colors of small objects contrasting with the background and recognized most readily by their color. More than this, red and yellow are the usual colors of such important objects as ripe fruits, domestic animals, wild animals to be hunted or avoided, blood and flesh. It is as animal colors, and particularly as mammalian colors, that red and yellow, with their darker shades, are most important to primitive man; and it would be hard to point out any equal importance of green and blue. Particularly in distinguishing cattle from one another, for purposes of barter, or for many other purposes of the herdsman, is it necessary to use the color as a mark, and to designate an individual by its color. If cows had affected the blues and greens, the history of color vocabularies would probably have been

quite different. It is also true<sup>1</sup> that the most accessible and most used pigments are red and yellow, and the use of pigments might easily give rise to a variety of objects alike save in color and needing to be designated by reference to their color. Probably the exact history of names for red and yellow has been different in different tribes.

Needs for the names of red and yellow are thus not far to seek, but the case is different with green and blue. It would be hard to show wherein primitive man suffers inconvenience from lack of names for green and blue. He needs, indeed, to observe and speak of the difference between growing and ripe or dead vegetation, but names for ripe and unripe, fresh and dried, living and dead supply this need admirably, and have the advantage of sticking close to the inner conditions indicated by the color signs. Similarly, primitive man needs to observe and speak of the difference between a blue and a gray sky, but weather names supply this need. We ourselves abandon our color names when speaking of the sky in a practical connection. We are unlikely to refer to it as deep blue, or grayish blue, or mottled blue and gray, or uniformly gray, when what we mean is that it is clear, hazy, fair or overcast. So long as vegetation and the sky are the only objects in which the colors green and blue are of practical importance, no motive can be assigned for the use of special names for these colors. And it is hard to think of other objects of these colors—especially of blue—which are important in primitive conditions. With the introduction of green and blue paints and dyes, these colors become important marks in distinguishing household objects; and it is probably owing to the use of pigments that names for green and blue have become stereotyped in European languages.

<sup>1</sup> Cf. Rivers, *Pop. Sci. Mo.*, 1901, Vol. 59, p. 56.

## PSYCHOLOGICAL LITERATURE.

### A REVIEW OF THE RECENT LITERATURE ON THE PSYCHOLOGY OF SEX.

Although the past few years have witnessed the appearance of a number of comprehensive and even encyclopedic works on various aspects of the problem of sex, there has been comparatively little advance in knowledge of the mental characteristics of sex. The few positive contributions in the literature of anatomy and experimental psychology I will review first, and will then deal briefly with the more pretentious treatises which are summaries and theoretical discussions.

The most important single contribution to our knowledge of the facts of the case is to be found in Dr. Franklin P. Mall's paper 'On Several Anatomical Characters of the Human Brain Said to Vary According to Race and Sex, with Especial Reference to the Weight of the Frontal Lobe' (*Am. J. of Anat.*, IX., p. 1, 1909). Dr. Mall's general conclusion is that there is as yet no reliable evidence for the variation of anatomical characters with either race or sex. The belief that the brains of females differ from those of males has been widely accepted, and has been thought to be conclusive evidence of the permanent inferiority of the female mind. The points in which the female brain has been said to be inferior to the male are: (1) total weight; (2) proportionate weight of the frontal lobe indicated by (a) actual weighings, and (b) determinations of the position of the central sulcus; (3) the area of the corpus callosum; (4) the complexity of gyri and sulci; (5) the conformation of gyri and sulci; and (6) the rate of development of the cortex in the fœtus. Dr. Mall's paper gives a critical review of the literature of the subject to date, and adds important new data.

It is now a generally accepted belief that the smaller gross weight of the female brain has no significance other than that of the smaller average size of the female. With regard to the other anatomical characters enumerated, Dr. Mall shows that those observers who have found differences characteristic of sex have been guilty of serious errors in scientific procedure. They have based conclusions on too small a series of observations, have used methods too crude to make anything but large and constant differences (which they did not obtain) significant, have made their determinations *with a knowledge of the sex of*

*the brain under consideration*, and have even, in some cases, drawn conclusions not justified by their own data. Those observers who have avoided these errors have found no differences characteristic of sex. Dr. Mall himself finds none, and is inclined to believe that they do not exist. The only exception to this statement is that he finds some evidence of a greater tendency of the male brain to vary from the normal conformation of gyri and sulci.

Waldeyer in his paper 'Ueber Gehirne menschlicher Zwillings- und Drillingsfrüchte verschiedenen Geschlechts' (*Zeit. f. Ethnol.*, 1908, Bd. 40, S. 262-272) agrees with Dr. Mall that it is by no means a general law that the male brain in twin fœtuses of different sex is further advanced in the formation of convolutions than the female. The male fœtus is frequently, though not always, larger than the female; and in case it is larger, usually has a more developed cortex.

Experimental psychology has yielded a few scattered researches bearing on the psychology of sex. They deal with (1) motor functions, (2) sensory processes, and (3) intellectual processes. Wells, 'Sex Differences in the Tapping Test' (*Amer. J. of Psychol.*, 1909, pp. 354-363) carried out a series of experiments in tapping on a telegraph key, in which he used ten men and ten women, attendants in an insane asylum, as subjects. He found the men somewhat superior to the women in the rate of tapping, but detected no difference between them in fatigue—a fact probably explained by the very slight amount of fatigue involved. (See Thompson, *Mental Traits of Sex*, p. 14, where the test involved much more intense fatigue.) He found a greater difference in the efficiency of the two hands in women than in men. (Compare below, Schuyten.) The women Wells found on the whole more variable than the men—a heresy which he attributes to their greater affectability. Schuyten, 'Bijdrage tot de Kennis der Rechts- en Linkshandigheid van de onderste leden' (*Paedologisch. Jaarboek*, 10, S. 42-51, 1908), found that in both sexes the left leg preponderates over the right in length and in development, but the difference is greater in men than in women, greater in women than in children. Miss Downey, 'Judgments on the Sex of Handwriting' (*Psychol. Rev.*, XVII., p. 205, 1910), investigated sex differences in the complicated function of handwriting. Her method was to collect 200 envelopes addressed by educated men and women, 100 by each sex. These were sorted according to sex by thirteen people. The percentage of right judgments was about the same as in Binet's tests. It ranged from 60 to 77 per cent. If only those judgments are recorded of which the observer felt certain, the

percentage is raised materially. The characteristics of handwriting on which judgments of sex were based were that the typical feminine hand is colorless, conventional, neat, and small, and frequently shows signs of unaccustomedness; while the typical male hand is 'bold, or careless, or experienced, above all individual.' In the case of ten men and seventeen women there was an inversion of the sex signs, leading to constant false judgments. Of the ten men, at least two had done very little writing, and four others were teachers — a profession which fosters a conventional hand. Of the seventeen women, at least half had been accustomed to more than a usual amount of writing. Miss Downey concludes that the sex signs of handwriting are of social rather than psychophysiological origin. They are largely determined by (1) the amount of writing done, (2) age and (3) professional requirements.

The only contribution I have been able to find dealing with any phase of sensory processes in relation to sex is the article by Winch on 'Colour Preferences of School Children' (*British J. of Psychol.*, III, p. 42, 1909). His method was to ask each of 2,000 school children to write the four primary colors, black and white in the order of preference. The results were then tabulated according to age, sex and position in school. Winch found constant differences correlated with sex which he considers characteristic, but he makes no attempt to interpret them. An examination of his tables shows that the like points between the two sexes are far more striking than their differences. In both cases blue ranks first and red second, while black is last. The sexual differences are that very young boys sometimes rank red first, while girls never do. White is somewhat more popular with girls than with boys, while green and yellow are a trifle more popular with boys than with girls. It is at least a very plausible guess that white gains its greater popularity among the girls by virtue of white dresses, their gala attire. The trifling displacements of green and yellow in the girls' records as compared with the boys may easily be explained by the higher rank of white. To speak of genuine sexual differences in color preference on the basis of these tests seems far-fetched.

In the realm of intellectual processes recent experimentation has dealt only with association. Of the two studies of association, I will deal first with the more detailed, though it was published later. Wrenschner, 'Die Reproduktion und Association von Vorstellungen' (*Zeit. f. Psychol.*, Ergänz. III, 1907-1909) makes elaborate comparisons between men and women, the cultured and the uncultured,

and children and adults — all on results obtained from twenty-two individuals. He examined two children, two servant maids, three working men, five educated women, and ten educated men. Although generalizations based on such an exceedingly small number of subjects, and frequently on trifling differences in the data, seem of very doubtful value, I will state briefly his conclusions with regard to sex. He found that men have a shorter reaction time for association than women. There are, as far as I know, no other tests made by the reaction time method. Tests made by requiring the subject to write a series of associations to a given word (see Thompson, *l. c.*, p. 131) have yielded contradictory results. With regard to qualitative differences in association, he makes the following statements. Men preponderate in the number of symmetrical reactions, mediate associations, multiple associations, associations by content, tendency to visualize, experience of emotional coloring, tendency to use previous stimulus words in reacting, number of unintended answers, and reduction with practice of the tendency to repeat the stimulus word. Women preponderate in the number of formal associations, the number of long reaction words, the variety of answers to a given stimulus word, the number of failures to react, the number of mistakes in the apprehension of the stimulus, the tendency to individualize, to reject answers because of emotional coloring, and to experience a rivalry in answers. From these characteristics Wrenschner draws the following formidable set of conclusions with regard to the process of association in the sexes. Women as compared with men show abnormality of reaction, meager presentations, a less active flow of ideas, less variety in ideas, a greater frequency of formal associations, imperfect adaptation to the conditions of the experiment, a more concrete form of response, a more subjective attitude, more indecision, and a less active attitude toward the experiment. It would be a simple matter to take Wrenschner's own data and derive quite a different set of generalizations which would have at least as much value as those he has drawn. Emma Fürst, 'Statistische Untersuchungen über Wortassoziationen und über familiäre Übereinstimmungen im Reactionstypus bei Ungebildeten' (*J. f. Psychol. und Neurol.*, IX., S. 243, 1907), carried out association tests on 100 individuals, men, women, and children, with the purpose of examining family likenesses in the type of association. She, like several previous observers, found that women have a greater tendency than men to the predicative type of association — a statement with which Wrenschner does not agree. She found men more given to outer and women to inner associations, and that men preponderate

in the definition type of association. Like Wrenschner, she detects a more personal attitude in the women. She found more uniformity among the ideas of related women than among those of related men. If her table of results is formulated according to formal associations and associations by content, the result accords with Wrenschner's statement, though the difference between the sexes is too slight to be really significant. The two sets of results agree, then, in finding formal associations more frequent among women, and associations by content among men, and in detecting a more personal attitude on the part of women. With regard to the other topics discussed, either they disagree, or the two sets of results are not comparable.

Though there is no very recent experimentation on memory as correlated with sex, Max Offner, in his book *Das Gedächtnis* (Berlin, 1909), devotes a brief section to a summary of the experimental material. For the sake of the value of the book as a whole, one hopes that his account of experimentation on other phases of the memory problem is more accurate than that of this section. By means of omissions and misrepresentations he gives quite a wrong impression of the general trend of experimentation on the subject. He seems to have singled out the few cases in which males were shown to excel in some phase of memory, and ignored many of the cases in which the reverse was true. In quoting Lobsien's results he states the fact that boys were slightly more accurate in reproducing the exact order of a series, and omits to mention the fact that the general range of memory was shown to be decidedly better in girls. He also misrepresents Miss Thompson's results. She found memory distinctly better in women for both visual and auditory material, instead of a *geringe Ueberlegenheit der Mädchen auf visuellem Gebiete*, and is entirely innocent of the *stärkeres Abschweifen der Reproduktion vom Gegenstand* on the part of the women with which she is credited.

In an article in the *American Journal of Psychology* (XXI., p. 114, 1910) on 'Spontaneous Constructions and Primitive Activities of Children Analogous to those of Primitive Man,' Acher draws some comparisons between the activities of boys and girls, based on the answers to a questionnaire. He says that boys are more interested than girls in points and edges because of their use of knives and sticking instruments; both sexes collect string, but they use it for different purposes — boys to tie hard knots, fish, and fly kites; girls to tie decorative knots, crochet, and make ornaments; girls are more apt than boys to attempt to modify bodily form; boys show a much greater interest than girls in the use of the whip and in throwing. The in-

terpretation of such observations is obviously sociological rather than psychological.

The most important of the comprehensive discussions of the question of sex are those of Iwan Bloch, *The Sexual Life of our Times; its Relation to Modern Civilization* (London: Rebman, 1909), and the work entitled *Mann und Weib. Ihre Beziehung zu einander und zum Kulturleben der Gegenwart* (Stuttgart: Union, 1908) edited by R. Kossmann and J. Weiss, and written by a dozen or more authors. Havelock Ellis makes another contribution to the field in the volume called *Studies in the Psychology of Sex*, Vol. VI., *Sex in Relation to Society* (Philadelphia: Davis, 1910). None of these works are to be considered important from the point of view of psychology, except for the fact that they and similar works contain the only psychological accounts of the sexual impulse itself which we possess. Such material is difficult to obtain and difficult to evaluate, and it will probably be a long time before it has much scientific value. The chief point of agreement among these writers is that the sexual impulse is much less intense in women than in men. Aside from this discussion, these works contain no original psychological data. Although it is impossible to discuss the problems of sexual physiology and hygiene, sexual ethics, the theory and practice of marriage, and prostitution, without at times adopting the psychological point of view, it is not the dominant one in any of these books. They belong rather in the field of sociology. The work of Kisch, *The Sexual Life of Women* (New York: Rebman, 1910), and of Mertens, *Das sexuelle Problem und seine moderne Krise* (München: Kupferschmid, 1910), are written exclusively from the physiological standpoint. They deal with problems of sexual hygiene, sexual disease, and, as a corollary, sexual education. It is well worth while in passing to remark that all of the group of books under discussion advocate more instruction for children in matters of sex. Most of them agree that such instruction should begin not later than the sixth year. There is a difference of opinion as to whether it should form part of the curriculum of the public schools. In addition to these larger works, there is the usual crop of magazine articles on various phases of the psychology of sex.

The general impression produced by a survey of this motley mass of material is first, that the literature of the subject is improving in tone. There is perhaps no field aspiring to be scientific where flagrant personal bias, logic martyred in the cause of supporting a prejudice, unfounded assertions, and even sentimental rot and drivel, have run riot to such an extent as here. It is very significant to note the im-

portance ascribed by Dr. Mall to the personal equation in so tangible and definite a problem as that of comparing the anatomical characters of the brains of the sexes. The outcome of problems where personal opinion has a still larger scope should be received with far more critical caution than heretofore. The signs in the literature of greater moderation in tone and more respect for evidence are in the direction of a much needed reform.

As to content, there seems to be a general trend toward the opinion that mind is probably not a secondary sexual character—in other words that there are probably few if any psychological differences of sex which are of biological origin—a statement which I think holds true in spite of the continued popularity of such books as Möbius' *Physiologischer Schwachsinn des Weibes* and Weininger's *Geschlecht und Character*. The tendency to minimize sexual differences is most marked with regard to intellectual processes, the field where most of the experimental work has been done, and in which the practical educational tests have been made. Even the time-honored belief that men are more capable of independent and creative work is beginning to give way in view of the successful competition of women in graduate work and in obtaining the doctorate (see Marion Talbot's *The Education of Women*, p. 21). The fundamental importance of sexual differences in affective processes and in standards of conduct still commands a larger measure of credence. The world at large is quite agreed that women are to a greater extent than men dominated by emotions, though the only direct experimental evidence does not support this view (see Thompson, *l. c.*, p. 137), and it is hard to reconcile with the attributes of patience, self-control, and power to endure pain, and with the much smaller share in the grand passion which are also ascribed to women.

The belief that there are fundamental differences in standards of conduct is less wide spread, and shows no such unanimity. It seems to part of the world quite certain that women have ethical disabilities consisting chiefly in an innate tendency to lie (Havelock Ellis, Weininger. See a refutation of the latter's arguments by M. Jörges, 'Geschlecht und Character,' *Zeitschr. f. Philos. u. philos. Kritik*, Bd. 135, S. 200, which takes them more seriously than they deserve), while to a long series of equally competent observers it seems quite certain that women are ethically superior to men, and that they may be counted upon to raise the moral tone of society! (For a recent exponent of this view see Wells, 'Some Questions Concerning the Higher Education of Women,' *Amer. J. of Sociol.*, 1909, p. 731).

Finally, one might characterize the drift of recent discussion as a

shift of emphasis from a biological to a sociological interpretation of the mental characteristics of sex. The very small amount of difference between the sexes in those functions open to experimentation, the contradictory results obtained from different series of investigations, and the nature of the differences which prove to be most constant, have led to the belief that the psychological differences of sex are of sociological rather than of biological origin. (See Densmore, *Sex Equality*, New York: Funk and Wagnalls, 1907; Alsberg, 'Die geistige Leistungsfähigkeit des Weibes im Lichte der neueren Forschung,' *Arch. f. Rassen- u. Gesell. Biol.*, IV., p. 476, 1907; Pelletier, 'La prétendue infériorité psychologique des femmes,' *Rev. socialiste*, XXIV., p. 45, 1908.)

Those who feel opposed to allowing women full opportunity of mental development have accordingly shifted the stress of their argument from the personal to the social standpoint. The cry is no longer that woman will injure herself by the mental and physical over-strain involved in the higher intellectual training, but that she will injure society by reducing her own reproductive activity (later marriages, fewer marriages, fewer children, opposition between intellectual and sexual functions), and thus lessen the chances of the best element to perpetuate itself (Alsberg, Wells, *l. c.*). The conclusion seems to be that it is the highest duty of woman to refrain voluntarily from developing her own intellectual capacities for fear of injuring society—a form of asceticism to which it is hard to subscribe. It seems possible that the higher education of women is being saddled with sins which belong by right to other phases of modern society, though this is not the place to discuss so complicated and difficult a problem.

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#### EDUCATIONAL PSYCHOLOGY.

*Educational Psychology*. Second Edition, Revised and Enlarged.

EDWARD L. THORNDIKE. New York: Teachers College, Columbia University, 1910. Pp. 248.

To the title of this work might well be added, for the benefit of the general psychologist, some such subtitle as 'The Study of Individual Differences.' For, though the book is intended largely for the use of students of educational psychology, its subject-matter is distinctly the study of individual psychology. Such topics as the psychology of learning are not here treated, but are reserved, as the preface indicates, for a second volume. The purpose of the present

volume is thus stated by the author: "This book attempts to apply to a number of educational problems the methods of exact science. The problems chosen are those of the mental natures of individual men, and the causes of their differences." The title chosen by the author should not, therefore, be allowed to obscure the fact that we possess, in this volume, a convenient outline of the methods and results of individual psychology, and also a critical discussion of the theoretical questions involved.

As compared with the first edition of 1903, the present book shows considerable rearrangement of form and much addition of substance. In the interest of unity of treatment former chapters on the effects of 'special training' and on the 'questionnaire studies' are now omitted. There have been other minor omissions and condensations, but the total size of the book has been increased, largely by references to recent investigations, and in part also by more extended discussion of certain moot questions.

The general course of the treatment may be summarized as follows: Beginning with a brief statement of the need for quantitative study of individual differences and of the difficulties of obtaining valid scales for mental measurements, the author proceeds to the problem of the causes of individual differences, and discusses in turn the influences of sex, race, immediate ancestry, maturity, and environment. He then passes to a discussion of the distribution of simple traits, of the combinations and correlations of simple traits and of the distribution of the resulting complex traits. Finally, there is a chapter on extreme individual differences, especially as observed in exceptional children.

To sex, race, maturity and environment the author is inclined to attribute less influence in producing intellectual differences than ordinary uncritical opinion would assign them. Comparative tests of the sexes have failed to show large differences on the average, and the dominant position of the male in intellectual and executive pursuits should be laid, apparently, in part to social conditions, in part to the fighting instinct and zeal for mastery of the male, and, very likely in largest part, to a greater variability of the male, which would cause the observed preponderance of males among idiots and also among individuals of the highest ability. The author's conclusions in regard to racial differences are very similar to these regarding sex. Maturity is of course a very genuine factor, yet the overlapping of individuals of widely different ages in tests is so great that native differences must be allowed to exceed in importance the differences due to age. In the

dispute between the supporters of heredity and environment, both sides are presented, and the general conclusion seems to be that heredity is the principal factor determining the relative standing of men, but that environment determines in large measure the absolute performance and also the special line of an individual's endeavor.

On the subjects of the distribution and correlation of mental traits, the author is so distinctly on his own ground, and also so much on controversial ground that his conclusions have a special interest. He holds to a continuity of mental variations, as opposed to the theory of types and of multimodal distribution. In simple traits, where measurement is relatively easy, results point clearly to continuity of distribution. "That continuity of variations in a mental trait taken singly is the rule can best be realized by trying to find exceptions to it. Such there may be, but I am not aware that any mental trait varying in amount has been shown to vary by discrete steps. A misleading appearance of regularly recurring gaps often arises from inadequate measurements. . . . A misleading appearance of irregular discontinuity often arises from the insufficient number of cases measured" (p. 149). On the other hand (p. 160), the author is not inclined to accept the 'normal' or probability distribution as being the necessary truth in every mental trait. He believes that though many of the causes which produce mental variations are small, chance and unrelated, some causes may be large in amount, or so correlated one with another that the resulting distribution of a trait may differ considerably from the 'normal.' At the same time, he would regard an observed divergence from the normal distribution as indicative of some special disturbing cause, for which it is best to search. A bimodal distribution should lead to a search for some large cause of differences, such as the presence or absence of special training. A skew distribution should lead to a search for some selective and eliminating influence.

The author devotes a separate chapter to the distribution of combined traits, and here again he has before him two extreme views for judgment. "On the one hand is the theory of multiple types, a theory which separates men more or less sharply into classes, and describes a man by naming the class to which he belongs. On the other hand is the theory of a single human type, a theory which joins all men one to another in a continuity of variation and describes a man by stating the nature and amount of his divergences from the single type" (pp. 195-197). As measurements of combined (or large and inclusive) traits are difficult and have not been satisfactorily carried out, a decision

between these rival theories cannot yet be reached. But to the author's mind the tendency of the known facts strongly favors the continuity doctrine here as well as in regard to the more measurable simple traits. To his mind the real foundation of the theory of multiple types is common linguistic usage, with its sharp distinctions between 'good' and 'bad,' 'quick' and 'slow,' 'objective' and 'subjective.' The whole tendency of quantitative studies is to throw doubt on sharp distinctions of this sort, or at least to show that individuals of strongly marked characteristics are exceptional rather than typical, and that a continuity of intermediate grades exists between any two opposed types. As evidence for this negative view of the type theory, the author cites the following points (pp. 200-201): (1) Such measurements as have been made (notably on imagery) in the expectation of confirming the multiple type theory have given results opposed to that theory. (2) The theory of multiple types implies an inverse correlation between desirable simple traits, but this is contrary to fact. (3) Since single traits are commonly distributed in an approximately 'normal' way, combinations of single traits must have the same general sort of distribution. (4) Investigators who are strongly imbued with the spirit of the multiple type theory (Meumann is cited) nevertheless find few actual instances in which the theory holds. (5) Investigators who set out definitely to demonstrate the theory (Stern is cited and his evidence scrutinized) are unable to bring forward a single clear case in its favor. All the supposed evidence is of very dubious nature, and much of it demonstrably false. The German investigators, in point of fact, have often been caught by the fallacy of studying too few individuals.

In the chapter on the correlations of mental traits the author makes use of Spearman's doctrine of attenuation, and therefore is led to revise considerably the conclusions stated in the first edition. He finds that the correlation between desirable mental traits, as far as tested, is practically always positive. There is no evidence of antagonism between any two such traits. "It is very, very hard to find any case of a negative correlation between desirable mental functions. Divergence toward what we vaguely call better adaptation to the world in any respect seems to be positively related to better adaptation in all or nearly all respects. And this seems specially true of the relations between original capacities" (p. 184). On the other hand, the author does not accept Spearman's doctrine of one basal mental function which forms the only common element in all sorts of intellectual activity. This doctrine seems inconsistent with the varying degrees

of correlation which appear between functions and between groups of functions. Nor does the author accept the existence of 'formal' functions, such as discrimination, memory and the like, because of the lack of sufficiently high correlation between performances thus formally related to each other. His conclusion is stated in the following words: "A table of the known degrees of relationship would abundantly confirm the statement that the mind must be regarded not as a functional unit, nor even as a collection of a few general faculties which work irrespective of material, but rather as a multitude of functions each of which involves content as well as form, and so is related closely to only a few of its fellows, to the others with greater and greater degrees of remoteness" (p. 187).

The book is characterized by a keen sense for facts as well as by insistence on adequate statistical treatment; and it is a book which should be considered not only by those who themselves make use of similar methods, but also by those who, distrusting statistics, believe themselves able to reach conclusions on these matters by some other path.

R. S. W.

#### MENTAL AND PHYSICAL TESTS.

*Manual of Mental and Physical Tests.* GUY MONTROSE WHIPPLE.  
Baltimore: Warwick & York, 1910. Pp. xx + 534.

The sub-title describes this work as "A book of directions compiled with special reference to the experimental study of school children in the laboratory or classroom." The object is partly to bring together scattered material, and partly to contribute to uniformity and standardization of procedure. There can be no doubt of the value of the book in both these respects. The collation of material includes a comparison of the various methods which have been employed for testing the same sort of function, and a brief summary of results so far obtained from each class of test—results concerning individual, sex and age differences, regarding the effects of practise and fatigue, regarding correlations with other tests and with school standing and general intelligence, and regarding numerous factors which may influence the individual's performance. These data regarding methods and results are systematically arranged under each test, so that the book becomes a convenient source of information for the teacher of psychology, even though he may not be conducting tests. Mention should also be made of the often extensive bibliographies appended to the several tests.

As regards standardization, it is far from the author's intention to put an end to the period of spontaneous variation in the evolution of

tests, but he believes that variations are likely to be introduced without sufficient cause, and so to make unnecessarily difficult the comparison of results. Now that the use of mental tests is becoming general, it must certainly be true that many investigations will be undertaken in which the gain from uniformity will much more than offset the loss of newly invented methods. The author aims at standardization (1) in the selection of tests, (2) in the procedure and conduct of each test, and (3) in the treatment of results. In the selection of a test, his plan is to present a survey of methods hitherto used for a certain purpose, and then, after criticism, to adopt one form of test, or a few forms, and describe these minutely, specifying apparatus—all of which can be obtained from a dealer in strict accordance with these specifications. The author is guided in the selection of a test partly by previous usage; where a test has already yielded a considerable mass of data for comparison, he tends to favor its further use. But inasmuch as many tests have hitherto been but slovenly in character, he is compelled to reject them, and sometimes to advocate new tests of his own devising. In these he aims at the greatest simplicity consistent with accurate results, but sometimes feels compelled to prescribe apparatus and experiments which are rather elaborate. In regard to the value of these original tests, the reviewer does not trust himself to speak in advance of experience; but he can see that each test has been worked over in a thoroughly first-hand manner.

Considerable emphasis is laid on the standardization of the instructions to the subject and of the whole conduct of the test. These matters are minutely prescribed; and it is certainly true that without uniformity here results become incomparable. The manner of treating the results is also prescribed in detail wherever this is justified by the state of the science. On the general statistical treatment of results, there is an introductory chapter, which presents (without mathematical deduction) the chief formulæ for computing means, variations, reliabilities, correlations and their attenuation. The use of the formulæ is exemplified by concrete instances of computation.

The range of tests described is indicated by the following (somewhat condensed) list: height, weight, cranial measurements, tests of muscular strength and endurance, of quickness, accuracy and steadiness of movement, of visual and auditory acuity, color blindness, discrimination of brightness, pitch, weight, pressure, measurements of the threshold for pain and of the two-point threshold, measurements of the span of apprehension with very brief and with longer exposures, cancellation, dot-counting and reading tests, tests of the ability to carry

on two operations simultaneously, of the reliability of testimony, of free and controlled association, of habit-formation and memory, of suggestibility, of invention, of the individual's vocabulary and range of information, and finally, the graded tests of De Sanctis and of Binet and Simon. In all, 54 tests or groups of tests are described.

R. S. W.

#### THE STUDY OF THE INDIVIDUAL.

*An Outline of Individual Study.* G. E. PARTRIDGE. New York: Sturgis & Walton Co., 1910. Pp. v + 240.

The author has written for teachers, in the hope of interesting them in the study of individual children. The book is a manual of methods of observation and experiment, but does not purport to prepare the reader for scientific investigation; rather it aims to lead him "to observe individuals more intelligently and systematically, and thus be the better able to understand and observe them." The author's experience in teaching Normal School students convinces him that some such study as this is the most useful form of psychology for the future teacher. One who enters on observations of individual children under the guidance of this book is not likely, as the author repeatedly emphasizes, to add to the store of scientific data, but will, it is hoped, come to a better appreciation of the problems of individuality, and incidentally detect the existence of exceptional traits which call for special educational treatment.

After introductory chapters on the general problems of the study of individuals, on the history of psychological tests, and on the biological conception of variation, the author proceeds to a broad survey of methods of testing and measuring individuals. He touches on anthropometry, clinical examination, motor and sensory tests, examination of the life of emotions and interests, and tests of memory and intelligence. In accordance, apparently, with his general plan, he seldom comes to close quarters with any test, and leaves details to the ingenuity of the reader. Little attempt is made to standardize procedure, and no instruction is given regarding the treatment of results. From the technical point of view, the most adequate chapters are those on the emotions and on purposive thinking, where technique has so far made little progress. Perhaps the most interesting item in the book, to the psychologist, will be a list (pp. 106-111) of about 800 English adjectives expressive of mental traits in which individuals differ. The author also reports an original study (pp. 201-220) of a pair of twins, in which an attempt is made to show the interrelation of the traits peculiar to each individual.

R. S. W.

*Abstracts of Lectures on the Study of Individuality.* WILLIAM STERN. Amer. Jour. of Psychol., 1910, XXI., 276-282.

The chaotic condition of the points of view and tests employed by biographers, alienists, etc., for the study of individuality clearly indicates the need of a 'Scheme of Psychography' which should, by inclusion, be neutral to the needs of the psychologist, alienist, historian, artist, man of science, and the educationist, and which should be worked out by the coöperation of many workers of many professions. A beginning has recently been made by the Commission for Psychography (*Zeitschrift f. angewandte Psychologie*, III., Heft 3). The trend of their scheme is as follows: When an individual is to be 'psychographed,' a sharp distinction must be made between the 'attitudes' to be observed directly and the 'characteristics' to be inferred from them. Attitudes are classified as experimental and natural, the latter appearing as reactions to definite stimuli occurring in the course of life, e. g., money, affairs, extraordinary circumstances. Aetiology and Symptomatology are also to be included in the scheme. Under the former head come data with reference to inheritance, diseases, character of the family, influences of nurture and education, etc., and under the latter data with reference to physical form, body mass, physiognomy, expressive movements, voice, etc.

A plea is made for the study of the individuality of the child, making especial use of complex functions, and the first six years are considered to be the most favorable period for such study. Binet's Intelligence Tests are strongly recommended for use in grading school children according to endowment, and a plea is made for the early diagnosis and special treatment of the 'supernormal' child.

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#### FREQUENCY TABLES FOR FREE ASSOCIATION.

*Associative Massenversuche.* GERTRUD SALING. *Zeitschr. f. Psychol.*, 1908, XLIX., 238-253.

*Beiträge zur Associationslehre auf Grund von Massenversuchen.* FERDINAND REINHOLD. *Ibid.*, 1910, LIV., 183-214.

*A Study of Association in Insanity. Part I. Association in Normal Subjects.* GRACE HELEN KENT and A. J. ROSANOFF. *Amer. Journ. of Insanity*, 1910, LXVII., 37-96.

The method employed in these three investigations is essentially the same: in a free association test, the same familiar words are pre-

sented as stimuli to each of many subjects, and the frequency of the various reaction words to each stimulus is determined. The results are presented in the form of an 'association lexicon,' which shows for each stimulus word all the words given in response, with the frequency of each. The work of Saling must be regarded as preliminary in character, since the number of persons examined was too small to permit the establishment of valid frequency tables. The work of Reinhold was done with school-girls, 30 from each of ten successive grades. The work of Kent and Rosanoff is much more extensive, since 1000 individuals (mostly adults, but including 150 high-school pupils and a few younger children) were tested, and 100 carefully selected words were used as stimuli. The resulting frequency table of these last authors, covering 49 closely printed but conveniently arranged pages, is a valuable addition to the apparatus of individual psychology, since it makes possible a new use of the free association test. It is now possible, by use of these authors' list of 100 stimulus words, and by reference to their tables, to determine for each individual tested the number of times he gives a frequent or common response, and the number of times he gives an infrequent or even an individual response (an individual response being defined for the user of this table, as one not made by any of the 1000 individuals included in the table). It is further possible to determine for each individual the average frequency of his responses, and so to assign his deviation from the mean of his social group. The meaning of this measure, *i. e.*, its correlation with other tests and estimates, remains to be worked out. From the results of Reinhold it appears that children, with increasing age, tend towards the more common responses. From the results of Kent and Rosanoff it appears that persons of collegiate education, as compared with those of only common school education, tend slightly toward the less common responses. It should be noted that each person was tested separately, and that the instructions emphasized the necessity of answering with a single word, and with the *first* word suggested by the stimulus word. Under these conditions, it appears that a tendency to individual reactions is usually a symptom of eccentricity and often of mental abnormality. Whereas the average per cent. of individual responses among the 1000 normal subjects was 6.8, insane patients often give more than 50 per cent. of individual reactions. A further report on the application of this test to the insane is promised by Kent and Rosanoff. Saling and Reinhold call attention to the importance of such frequency tables in the use of the association test for the detection of emotional 'complexes' and of special or criminal knowledge.

As illustrations of the frequency of different responses, the following are cited from the table of Kent and Rosanoff.

To the stimulus word, *Table*, 267 individuals reacted with 'chair'; 76 with 'wood'; 75 with 'furniture'; 63 with 'eat'; 57 with 'cloth'; 40 with 'dishes'; 36 with 'stand'; 34 with 'eating'; 29 with 'food'; 26 with 'dinner'; 17 with 'cover'; 14 with 'board'; 13 with 'leg' and 10 with 'legs'; 11 with 'desk'; 10 with 'round'; and 222 with words having a frequency of less than 10 in the 1000.

To the stimulus word, *Dark*, 427 reacted with 'light'; 221 with 'night'; 76 with 'black'; 28 with 'color'; 22 with 'room'; 15 with 'bright'; 11 with 'gloomy'; and 200 with words having a frequency of less than 10 in the 1000.

R. S. W.

#### MENTAL IMAGERY.

*The Distribution and Functions of Mental Imagery.* GEORGE HERBERT BETTS. New York: Teachers College, Columbia University, 1909. Pp. 99.

In this study of the correlations of imagery, the author has employed a more thoroughgoing questionnaire than has been customary; he has sought to secure a range of questions that shall be equally fair to all the senses, and to call for immediate judgments on the vividness of many different images, grading each on a scale of seven degrees. This method makes it possible to treat the results by statistical methods to a greater extent than has hitherto been done. The author is specially interested in correlations; he verifies the decrease of vividness of imagery with increasing age, as originally reported by Galton, and the slight but apparently negative correlation of vividness with success in college studies, as originally reported by Armstrong. His most important results are however (1) a much greater equality between images appertaining to the various senses, as regards their vividness, than has usually been asserted, and (2) a decided *positive* correlation between the vividness of imagery of the different senses. For many years, the belief of the psychological world, founded largely on a few cases of high specialization in some department of imagery, has been in favor of a *negative* correlation. An individual has been assigned to the class of the 'eye-minded' or the 'ear-minded,' etc. True, those who, like Meumann, have investigated a considerable number of individuals have become convinced that 'pure types' were rare, at least among young persons; yet the general opinion has remained that vividness of imagery of one sense is asso-

ciated, in any individual, with a tendency to lack of vividness in other senses. The results of Betts are certainly the most adequate so far published for examining this question, and the positive correlation is clear enough to settle the question. The Pearson coefficient of correlation, when calculated as between the vividness of imagery of each sense with that in each other sense, averaged as follows: In a class of 34 college students of psychology,  $+0.68$ ; in an 'extension' class of 45 students, mostly school teachers,  $+0.43$ ; and in a group of 18 'psychologists,' comprising teachers and graduate students of the subjects,  $+0.79$ . Now there is no doubt one reservation to be made: since it is impossible to compare directly the images of different individuals, the result is dependent on each individual's conception of the meaning of a vivid image and of the scale of vividness; and it is easy to show, in untrained subjects, that this conception is subject to considerable error. Thus, the reviewer has found individuals who, on first answering Galton's questionnaire, assigned to their images the highest degree of vividness, but who, after further discussion, actually reduced their grade to near the zero of the scale. With this difficulty in mind, we might be inclined to read Betts's result as simply indicating that individuals differ in their resistance to the natural tendency to rank themselves high in imagery. But this suspicion cannot be upheld in the face of the specially high correlation existing within the expert group. If any of the existing testimony regarding imagery has scientific value, that secured by the present author is certainly entitled to precedence, and the positive correlation appearing in his results must be regarded as established till very good evidence is presented to the contrary.<sup>1</sup>

R. S. W.

#### TESTS OF GENERAL INTELLIGENCE.

*Experimental Tests of General Intelligence.* CYRIL BURT. Brit. J. of Psychol., 1909, III., 94-177.

This is the report of an admirable investigation on the measurement of 'general intelligence.' Its original purpose was to test the mathematical methods of Spearman, and to verify the experimental results of both Spearman and Meumann. Three specific problems were set: (1) Can the presence of general intelligence be detected and its amount measured? (2) "Can its nature be isolated and its meaning analysed?" (3) Is it predominantly an inherited quality or is it due mainly to environmental influences?

Twelve tests were employed which group themselves under five heads: *Sensory tests* (discrimination of two points upon the skin, of

<sup>1</sup>Cf. PSYCHOLOGICAL BULLETIN, March, 1910, pp. 88 ff.

lifted weights, of pitch, and of length of line by the eye), *motor test* (tapping and card dealing), *sensori-motor tests* (card sorting and alphabet finding), *association tests* (immediate retention of words and syllables, mirror writing, and 'progressive apperception'), a test of *voluntary attention* (dotting irregular dots). These tests were performed upon two quite different groups of boys, twelve and a half to thirteen and a half years old, from two Oxford boys' schools. One group of thirty boys belonged to an elementary school which is attended by sons of the lower middle class. The other group of thirteen boys came from a preparatory school which is attended by boys from educated and intellectual families. The tests were made, apparently with a great deal of care, upon each boy individually.

The general intelligence of the boys was estimated by three independent rankings, one by the headmaster, one by the classmaster and one by two school fellows. These three were then compiled into one list. Coefficients of correlation were computed, by Spearman's foot-rule method, between each test and general intelligence, also between each test and every other test. The following are the coefficients between intelligence and each test: dotting .85, progressive apperception .83, mirror .71, memory .71, alphabet finding .79, sorting .76, dealing .30, tapping .53, pitch discrimination .46, estimation of lines .47, lifting weights —.10. Six of these coefficients are below .50 and six above .50. The former are derived from the simple sensory and motor tests while the latter are the correlations between the intelligence and the higher mental functions. On the basis of these, Burt suggests that "by means of some half dozen tests, we are able independently to arrange a group of boys in an order of intelligence, which shall be decidedly more accurate than the order given by scholastic examinations, and probably more accurate than the order given by the master." With regard to the inheritance of intelligence it seems that the two groups of boys are too small to allow a valid comparison. The tests however show that "with two exceptions the average performance of the boys of the preparatory school are all superior to those of the boys of the elementary school."

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#### AN EXPLANATION OF HAMLET'S MYSTERY.

*The Œdipus Complex as an Explanation of Hamlet's Mystery. A Study in Motive.* ERNEST JONES, M.D. Amer. Jour. of Psych., 1910, XXI., 72-113.

The paper is an exposition of an hypothesis suggested by Freud in a footnote of his *Traumdeutung*. Hamlet's inability to avenge the

death of his father cannot have been due to a general weakness of will as his prompt and decisive actions in the case of Polonius and of Guildenstern and Rosencrantz bear witness. Nor were external conditions prohibitive. There was, rather, a special inhibition to this particular act. This may be traced to a complex involving childhood repression of sex affection toward his mother with accompanying jealousy of his father, the latter being concealed by a compensatory solicitude. Hamlet's inability to act is due to the dread of arousing to consciousness this submerged complex. Shakespeare is supposed to have had a similar hidden complex. "The new life which Shakespeare poured into the old tragedy was the outcome of inspirations that took their origin in the deepest and most hidden parts of his mind."

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H. A. RUGER.

#### THE IDEA OF THE SOUL.

*The Idea of the Soul.* A. E. CRAWLEY, M.A. London: A. and C. Black, 1909. Pp. 307.

In this book Mr. Crawley conceives that he has for the first time applied 'the verified experimental results of psychology' in the interpretation of the facts concerning the animistic beliefs of savages made known by anthropology and folk-lore; and that from this marriage of the two sciences there legitimately issues a "solution of the most important and far-reaching of all sociological questions, the genesis of that great conception which divides man into body and soul, and the universe into matter and spirit." It is in this unpretentious manner that the author puts forward a new hypothesis concerning the origin and early development of the idea of the soul. He proposes to introduce 'exactness of method' into anthropology, and thereby to put an end to the discord of opinions which still prevails in that science. The soul, Mr. Crawley is convinced, did not owe its origin to the dream-experience, nor to hallucinations, nor to a tendency of early man to conceive of natural causation in anthropomorphic terms, nor to the personalization of the idea of 'vital energy'; all these explanations are for him not psychological enough. In its primal form the soul was nothing more or less than the memory image — "the sensory, chiefly visual, image in memory, of the whole personality or totality of the person or object." The idea thus has at once a simpler and a less dubious genesis than has often been ascribed to it; it is not the product of an illusion nor of any abnormal experience, but the primitive formulation of a psychological fact — namely, that the image is dis-

sociable from the percept though derivative from it, and that thus, through the power of memory, the mental world is numerically distinct and qualitatively different from the world of objects, from which its content is none the less drawn. From the contrast between the characteristics of the memory-image and those of the percept Mr. Crawley proposes to explain the well-known peculiarities of the soul in savage belief — its vagueness, tenuity, general similarity in form to the body, reduced size, duplicability, survival of bodily death, and the like.

The evidence for all this consists partly in an accumulation of ethnographic data, partly in reasonings concerning what 'must have been' the processes by which the primitive mind arrived at the soul-concept. Neither part of the argument is characterized by sound scientific method. The anthropological facts which Mr. Crawley adduces — with an abundance which will make his long chapter of over one hundred pages on 'pre-scientific psychologies' somewhat serviceable as a work of reference — are largely beside the mark. For, by Mr. Crawley's own confession, none of these psychologies belongs to the 'first stage of mental evolution' (p. 25); they are all removed by an indefinitely long development from the truly primitive. They therefore throw no direct light upon the problem of genesis; and the author scarcely even attempts a cogent proof that the several features of these soul-beliefs of savages actually known to us are such that they could conceivably have developed from no other antecedents than the one which his hypothesis recognizes. Furthermore, he jumbles together under the name 'soul' entities of the most diverse sort, which in some cases have nothing in common save the fact that European describers of savage beliefs have, *faute de mieux*, used the one name to designate them. There are, in particular, two notions which, for most races in the stage of savagery, seem to be tolerably well differentiated: the notion of the death-surviving spirit-double of the individual person or thing, and the notion of life-force (*mana* or *orenda* or *manitou*) or of the quantum of that all-important stuff which is allotted to an individual. The latter idea unquestionably tends among some races towards personification and towards the assumption of certain of the aspects of the spirit-double, but even where there is such a partial similarity, the two conceptions often remain significantly distinct. I cite only a single example of Mr. Crawley's unwarranted identification of the two. Among the Batak and other peoples of the Malay Archipelago, we learn from J. Warneck's valuable monograph (*Die Religion der Batak, ein Paradigma für animistischen Religionen des Indischen Archipels*, 1909), three ideas are plainly distinguished by

the terms *roha*, *begu* and *tondi*. The *roha* is, in Warneck's phrase, a man's *Personenbewusstsein*, his Ego; it is what thinks, feels, wills. The *begu* is the spirit which survives after death, usually to make much trouble for the living; the world is terrifyingly well-peopled with these restless and malicious spirits, and the most engrossing concern of the Batak is to elude, placate, or overpower the *begu*. Quite other is the *tondi*. It is defined as *Lebenskraft*, *Lebensmaterie*, or *Seelenstoff*. It has apparently several attributes of personality (the account is somewhat self-contradictory on this point), and during lifetime it is ordinarily 'in' the body. But primarily it is the source of power or skill or fertility; and it is characteristically thought of in quantitative terms. It is ascribable in some degree to almost everything, but some things and some persons have much more of it than others. It is present in exceptional degree in some parts of the body—the head, the liver, the blood. Chiefs have more of it than common men. It is transferable; the Batak warrior drinks the blood of a slain enemy to absorb the latter's *tondi*. Sickness means the loss of the *tondi*—which by no means implies loss of consciousness. At death, a man's *tondi* permanently leaves him, and what lives on, his shadowy self, is only the *begu*; his *tondi* now animates other beings. The two conceptions are, for the Batak, 'pragmatically' wholly different; and a separate genetic explanation must probably be sought for each. In the three pages which he devotes to the Batak, Mr. Crawley speaks of *tondi* simply as 'the soul,' and fails to make clear how essentially distinct are the several notions for which the one English word is employed. A similar lack of careful discriminations characterizes most of his use of anthropological material, and renders it impossible to assign any considerable weight to this part of his work.

The author's *a priori* arguments from psychological considerations are, however, scarcely more impressive. He assumes—what needs proving—that the significant and pregnant elements of the idea of the soul originated at a stage in mental evolution in which men were incapable of separating in thought the details of a percept from the percept as a whole, or of forming 'abstract' notions of generic qualities, or of regarding the objects presented in consciousness as being other or more than what they seem. On the ground of these assumptions Mr. Crawley rejects the dream theory and other rival hypotheses. But under such conditions the characteristic content of the soul-idea could just as little have arisen from the memory-image. Mr. Crawley sometimes seems to mean that a memory-image *is* the idea of the soul; thus dogs, to whom we have, I suppose, plausible

analogical reasons for ascribing visual and other imagery, would be classified among the animists. Such a view, however, is not an explanation of the genesis of the idea of the soul as spirit-double or as life-force, but only a queer use of language. It is only when the image ceased to be taken as a matter of course, when it was generalized into an 'abstraction' or made the starting-point for an inference, that it could have engendered any of those concepts and those beliefs which anthropologists have designated as 'the idea of the soul.' Not until reflection began vaguely to contrast image and percept, to consider the meaning of their relations of similarity and difference, and to infer that an objective counterpart of the former continues to exist even when *not* remembered by anybody else, could the transient memory-image have been transformed into the death-surviving double. If, therefore, other theories are to be rejected because they presuppose, for the explanation of the origin of the idea, mental processes that are 'late' in development, Mr. Crawley's own hypothesis must be given up on similar grounds. If, on the other hand, it be once recognized that any account of the genesis of the beliefs about 'the soul' which we now find among savages must credit the originators of those beliefs with some rudimentary powers of intellection, Mr. Crawley's explanation appears to have no advantage over others. It has, indeed, some peculiar difficulties of its own. One of these lies in the fact—explicitly recognized by Mr. Crawley (pp. 210, 250 f.)—that a man's visual memory-images are not primarily of himself but of other people. Hence, originally, A's soul would exist only by grace of B and C. Mr. Henry James's ingenious story *The Private Life* has a hero who, though endowed with all the social graces, and the observed of all observers when in company, lapsed into nonentity when no one was present to observe him. He had no private life. Thus, by Mr. Crawley's hypothesis, must it have been with the primeval soul. To discover that he himself, no less than B, had a soul, A must have depended upon B's report, or upon a somewhat complicated inference: "Since I sometimes see a vague, intangible double of B when B's tangible body is not present, B must similarly see a double of me when I (from my own point of view) am not present to B; *ergo*, I possess a soul." However this might have come about, we should at any rate expect that the eventual generalized conception of the soul would show some traces of the fact that souls originally lived and moved and had their being, not in the persons *of whom* they were souls, but in the minds of other persons. Why, again—if Mr. Crawley's hypothesis be true—is it that savage A does not consider that he is meeting the

soul of B whenever he visualizes B? Or is it Mr. Crawley's contention that the savage does so — that his very memory is at all moments infested with terrifying ghosts of other men and things, to which he ascribes just the same sort of effective reality which he ascribed to the grosser objects of perception of which the 'souls' are images? Some such consequences seem properly implied by the hypothesis; and they appear sufficient to reduce it to a good deal of an absurdity.

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#### MENTAL FUNCTIONS IN PRIMITIVE PEOPLES.

*Les Fonctions Mentales dans les Sociétés Inférieures.* L. LÉVY-BRUHL. Paris: Félix Alcan, 1910.

Bruhl's work appears as the third volume of the *Travaux de l'Année Sociologique* published under the direction of M. E. Durkheim. B. endorses the motto of the school of writers headed by that *savant*, that social phenomena have laws of their own (p. 2). The same applies to those 'collective representations' which lie at the root of social facts. B. sets out to determine the most general laws which govern such representations (p. 3). The interpretations of primitive life and thought given by Tylor, Frazer, Lang, and others, must be condemned, for they have disregarded the fundamental question: can the facts of primitive life be explained along the same lines as the facts of civilized life? (pp. 6-7). Led by the postulate of the psychic unity of mankind, these authors assume a general animism (p. 14). But that animism is invariably derived from the psychology of the individual. The resulting interpretation of primitive man's beliefs and institutions is necessarily artificial. If we want a firm foundation for our researches, we must remember that collective representations impose themselves upon the individual. They are for him a matter of belief and not a product of his reasoning (p. 16).

The rest of the volume falls into four parts. Part I. deals with "Les représentations collectives dans les perceptions des primitifs et leur caractère mystique." The representations of primitive peoples are not, B. believes, in a strict sense representations at all; for into the make-up of each representation enter emotional and volitional elements which form with it an indecomposable whole (p. 28). Not a single object is represented without such an emotional setting. For this reason the primitive man perceives nothing as we do (p. 37). Mystic properties form an integral part of his representations and cannot be excluded from them (p. 39). Thus an object and its material copy, for instance, are not regarded as one and the same thing

on account of a mental confusion, nor as a consequence of an animistic creed, but they are so regarded because the same mystic elements enter both (p. 44). Other well-selected illustrations from many peoples are adduced. Facts contradictory to his beliefs do not in the least shake the confidence of the primitive man, for these facts can always be explained away on the basis of these same beliefs (p. 61).

Primitive representations being different from ours, the connections between these representations are also different, for they are not guided by logic but by a mystic rapport or participation between beings and objects, etc. (p. 70). Thus it happens that sequences in experience which to us seem all-important, are overlooked, and, on the other hand, sequences which never actually occur are believed in (pp. 74-75). This stage in man's mentality may thus be designated as *prelogical* although it need not for that reason be either antilogical or allogical (p. 79). The distinctions between one and many, in direct contact and at a distance, etc., are often not made, in this stage (p. 81). These considerations lead B. to attack Tylor's doctrine of a universal belief in a soul as expressed in his animism. At the hand of a not very extensive but well chosen material, B. demonstrates that the prevalent belief of primitive peoples is in several souls, not in one soul or double. These souls, moreover, are concepts of such indefiniteness and instability as not to be really comparable to our concept of soul (pp. 81, 82, 83). The conclusion arrived at by B. at this stage of his enquiry is worth quoting in full: "Orientée autrement que la nôtre, préoccupée avant tout des relations et des propriétés mystiques, ayant pour loi principale la loi de participation, la mentalité des primitifs interprète nécessairement d'une façon différente de la nôtre ce que nous appelons la nature et l'expérience. Elle voit partout des communications de propriétés, par transfert, par contact, par transmissions au loin, par contamination, par souillure, par possession, par une multitude d'opérations, en un mot, qui font participer, instantanément ou au bout d'un temps plus ou moins long, un objet ou un être à une vertu donnée, — qui le sacralisent, par exemple, ou le désacralisent au commencement et à la fin d'une cérémonie" (p. 104). The collective representation which underlies this mentality is a belief in a mystic *continuum* which pervades nature. Personalities, souls, spirits, are of later origin (pp. 109-110).

The mechanism of prelogical mentality is particularly differentiated from that of our own in that the connections between representations are, in that stage of mentality, given with the representations themselves. The latter are not isolated and analyzed, as they are with us,

to be then logically connected. "Elles sont toujours engagées dans des préperceptions, des préconceptions, des préliations, on pourrait presque dire des préraisonnements : et c'est ainsi que cette mentalité, précisément parce qu'elle est mystique, est aussi prélogique" (pp. 114-115). When each object has a mystic significance, abstraction must obviously be reduced in its functions; memory, on the other hand, becomes the dominant factor (pp. 123 *et seq.*). Some pages are devoted to illustrations of the marvellous development of that faculty among primitive peoples. Abstraction and generalization, although exercised, are thoroughly different from ours, for again the mystic connections are the determining factor; as, for instance, among the Huichol who assert that the deer is a feather because the former is possessed of the same mystic qualities as are the latter (p. 135).

We should expect to find the distinctive qualities of primitive mentality reflected in language. (Part II.) Thus we discover that instead of one plural a great many primitive languages have separate forms to designate various kinds of plurality (pp. 153 *et seq.*). The number of verbal forms in some of these languages is enormous; the same is true of adverbs, especially of those indicating spacial relations. B. endorses Gatschet's conclusion that the categories of position and distance are as important for the representations of primitive peoples as the categories of time and causality are for us (p. 165). The great variety of verbal suffixes and affixes, especially of those connected with verbs designating motion, is mentioned and illustrated in the same connection (pp. 165 *et seq.*). The common tendency of primitive languages is not to render the general impression received from an object, but to designate its exact shape, outline, position, motion, mode of activity in space, etc. In this characteristic, primitive speech approaches sign-language used by so many primitive peoples. In the pages that follow B. attempts to draw a close parallel between those two types of expression (pp. 175 *et seq.*). All objects have mystic properties, and the spoken word used to designate objects and actions has mystic properties of its own (pp. 199 *et seq.*).

In the concluding chapter of this part, number is treated very much in the same way as speech was in the preceding chapter (pp. 204-257).

Part III. is devoted to the "Institutions ou sont impliquées des représentations collectives régies par la loi de participation." A series of illustrations is brought before the reader showing the magical processes employed in the chase (pp. 263-275), at fishing (pp. 275-281), in war (pp. 281-283), as well as the magical ceremonies intended to increase the food-supply, or in other ways to influence the

course of nature (pp. 283-296). Further examples illustrate the mystic rapport between husband and wife, parent and child (pp. 296-304). The art of the medicine-man is discussed under the same heading, followed by a few pages devoted to divination and magic proper (pp. 304-352). An entire section is devoted to the relations between the living and the dead, in which B. tries to demonstrate that the concepts of life and death are among primitive peoples by no means as clearly differentiated as they are with us (p. 358); and, on the other hand, these concepts are full of mystic content which determines the character of the practices at death, burial, birth, initiation, etc. (pp. 352-421).

The fourth and last part is short. In it B. indicates in general outline the "*Passage à des types supérieurs de mentalité.*" No sharp line can be drawn between prelogical and logical mentality. The former is not utterly foreign to the most elementary logical connection; while the latter, as we find it among the most civilized peoples, always retains representations, collective in character, which rest on a more or less disguised mystical background. The transition from one mentality to the other occurred gradually. As mystic participations tended to become represented, the objective character of experience came gradually to the fore. Concepts crystallized into definite and objective categories. But while man's mentality was thus becoming transformed, interpretations and reinterpretations of concepts were made, as for instance in myths, which renders the task of reconstructing the course of this evolution an almost hopeless one (p. 440).

B. discards the doctrine of the psychic unity of mankind. If primitive society is to be understood, its life and soul must be interpreted along the line of collective mystic representations, a radical difference between the two mentalities being a necessary assumption. B. regards his own work, together with the works of Durkheim, Huber and Mauss, and some others, as the first attempts in that direction (pp. 425-455).

An interesting book might be written in answer to Professor Lévy-Bruhl. We doubt whether he has always correctly compassed the bearing of his argument. The mystic character of primitive feeling and thinking is laid bare by the research of modern ethnology. But is this mysticism all-pervading? Does not the primitive man himself draw distinctions between the mystical, the powerful, dangerous, sacred, and the ordinary, objective, indifferent reality? Many facts could be adduced to indicate that he *does* draw this distinction. Again, the prelogical character of primitive mentality is ably demon-

strated by B. But is the whole of primitive mentality thus prelogical? Are not many of its conclusions arrived at by mental processes as logical as our own, *some* of our own?

A still more fundamental objection must be made. Is the procedure of B. quite logical in so far as he brings the collective representations of primitive man into juxtaposition with individual mental processes of the civilized? If he were to take our own collective representations, with the far from logical character of their content and connections, a comparison with primitive mentality would not yield results so unfavorable to the latter. It seems also that B. rather weakens his position by his attack on the doctrine of the psychic unity of mankind. For this doctrine applies to man as an individual who, under similar physical and social environment, would presumably act the same all over the world. The social environment of primitive communities differs from that of the civilized; hence the mentality, which is mainly a product of social environment, is different. B.'s attitude on this question reminds us that the veil which rests on the relation of the individual to the social factor in society is as yet far from lifted.

As a whole, however, the book is an excellent one and may even be considered remarkable. The rationalistic tendency which still pervades so many works on primitive society is categorically set aside by B. He duly emphasizes the great complexity of the factors which coöperate in the development of any institution or any body of beliefs. He challenges with great force the monopoly of the doctrine of animism in doing service as a universal principle of interpretation of primitive belief. Nor does he deal any more gently with the associationist interpretation of magic. He emphasizes, finally, the great importance of the collective factor in all matters social and most matters individual. The numerous illustrations of the customs and beliefs of savages are drawn from the most reliable sources.

Needless to say, the anthropologists will join the psychologists and the sociologists in their impatience for Professor Lévy-Bruhl's next contribution to the subject of the mentality of primitive man.

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A. A. GOLDENWEISER.

#### ANTHROPOLOGY.

*Psychological Problems in Anthropology.* FRANZ BOAS. Amer. J. of Psychol., 1910, XXI, 371-384.

Professor Boas indicates a number of fields in which anthropological inquiry brings back data of interest to the psychologist. The

attempt to gain knowledge of the events by which the human race has formed its past and present types is closely involved with the inquiry into the psychological laws of general application among mankind, and of how these laws may differ among different groups. That there are deep seated mental differences between such groups is perhaps correlated with apparent differences in their nervous systems,—though there is no justification for holding that one set of characteristics is a special advance over another. The frequent assumption of general similarity of mental reaction among societies of similar structure is interestingly criticized. The universal idea of existence after death may in reality be the product of quite different mental processes, according, *e. g.*, to whether the belief is that the soul is reborn in another child of the same family, or enters into an animal, etc. The totemisms—beliefs in supernatural relationship between the family and certain objects and animals—may have a very different psychogenesis, according, *e. g.*, as the family may be the descendant of the animal, or may have acquired his friendship and protection through special external circumstances. Among the differential mental reactions of primitive man especial mention is made of animism, of the ill-drawn distinction between man and beast, and the tendency to raise the attribute to the level of an entity. There is some interesting linguistic discussion on this point. Further, among the more evolved peoples decorative art serves an almost exclusively esthetic purpose; among primitive peoples there is apt to be a highly developed symbolism, with the decorative function relegated far into the background. In speaking of the psychology of customs it is brought out that while the automatic habitual actions have little value for consciousness and for affective reaction, the performance of opposed actions is apt to be difficult and accompanied by considerable affective displeasure. What the child does not learn by unconscious imitation he thus learns by positive correction from his elders, and it is through this process that the habit may come into consciousness as a recognized custom. The taboo may originate in a series of circumstances that long render impossible the act tabooed. The phenomenon of secondary explanation finds abundant illustration in primitive as well as in civilized life; one group may borrow the art of another and attach to it a very different symbolism; and myths of identical source may be made to explain now a particular characteristic of its subject, now the origin of some natural phenomenon. These are some of the topics regarding which fruitful psychological investigation is possible in anthropological territory.

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## BOOKS RECEIVED DURING SEPTEMBER.

*Trick Methods of Eusapia Paladino.* STANLEY LEFEVRE KREBS. Philadelphia, Reprinted from the Reformed Church Review, XIV, 1910. Pp. 337-383.

*Die Bestrafung der Motive und die Motive der Bestrafung. Rechtsphilosophische und kriminalpsychologische Studien.* JULIUS FRIEDRICH. Berlin und Leipzig: Walther Rothschild, 1910. Pp. 307.

*Bulletin No. 2. Government Hospital for the Insane.* WILLIAM A. WHITE. Washington: Government Printing Office, 1910. Pp. 133.

*Ueber den Traum.* 1. Bd. J. MOURLY VOLD. Leipzig: J. A. Barth, 1910. Pp. xii + 435. Mk. 11.

*Ueber Annahmen.* A. MEINONG. (2. umgearb. Aufl.) Leipzig: J. A. Barth, 1910. Pp. vi + 403. Mk. 10.

*Grenzen und Ziele der Wissenschaft.* HUGO DINGLER. Leipzig: J. A. Barth, 1910. Pp. 125. Mk. 2.50.

## NOTES AND NEWS.

DR. BIRD T. BALDWIN, who was for the past year a lecturer in education and psychology at the University of Chicago, has accepted a call to become associate professor of education and head of the School of Practice Teaching in the University of Texas.

THE following promotions have been made in the Department of Psychology of the University of Pennsylvania. Dr. Arthur Holmes and Dr. F. M. Urban have been advanced from instructorships to assistant professorships. Mr. S. W. Fernberger has been advanced from the position of assistant to that of instructor in psychology.

PROFESSOR MAX MEYER, of the University of Missouri, has returned from a year's leave to Europe. Dr. W. H. Pyle, who was in charge of Professor Meyer's course during his absence, has been retained as instructor in educational psychology.

PROFESSOR PILLSBURY, of the University of Michigan, has been advanced from a junior professorship to a professorship of psychology.

THE present number of the Bulletin, dealing especially with race and individual psychology, has been prepared under the editorial care of Professor R. S. Woodworth.

